NRF REVIEW

ARIMA Procedure

Conditional Least Squares Estimation

Parameter	Estimate	Approx. Std Error	T Ratio	Lag
MU	4.77806	0.76450	6.25	0
AR1,1	0.22152	0.17032	1.30	1
AR1,2	0.38657	0.17501	2.21	2

Constant Estimate = 1.37255556

Variance Estimate = 3.48469615
Std Error Estimate = 1.86673409
AIC = 137.701275*
SBC = 142.190798*
Number of Residuals= 33
* Does not include log determinant.

NRF REVIEW

ARIMA Procedure

Correlations of the Estimates

Parameter	MU	AR1,1	AR1,2
MU	1.000	-0.092	-0.192
AR1,1	-0.092	1.000	-0.347
AR1,2	-0.192	-0.347	1.000

NRF REVIEW

ARIMA Procedure

Autocorrelation Check of Residuals

To Lag	Chi Square	DF	Prob	Autocorrelations					
6	8.08	4	0.089	-0.093	-0.111	0.163	0.217	0.185	-0.265
12	12.78	10	0.236	-0.026	0.228	0.005	-0.161	-0.130	-0.036
18	17.00	16	0.386	0.059	-0.192	-0.088	-0.075	0.062	-0.090
24	19.53	22	0.612	-0.018	-0.078	-0.027	0.107	-0.077	-0.010

NRF REVIEW

ARIMA Procedure

Model for variable USPRICE

Estimated Mean = 4.77806451

Autoregressive Factors
Factor 1: 1 - 0.22152 B**(1) - 0.38657 B**(2)

NRF REVIEW

ARIMA Procedure

Name of variable = TELECOM.

Mean of working series = 4.671212
Standard deviation = 3.871392
Number of observations = 33

Autocorrelations

Lag	Covariance	Correlation	-1 9 8 7 6 5 4 3 2 1 0 1 2 3 4 5 6 7 8 9 1
0	14.987677	1.00000	|**********|
1	4.213701	0.28114	|******. |
2	2.372121	0.15827	|*** . |

"." marks two standard errors

ARIMA Procedure

Inverse Autocorrelations

Lag	Correlation	-1 9 8 7 6 5 4 3 2 1 0 1 2 3 4 5 6 7 8 9 1
1	-0.21879	| . **** | . |
2	-0.08014	| . ** | . |

Partial Autocorrelations

Lag	Correlation	-1 9 8 7 6 5 4 3 2 1 0 1 2 3 4 5 6 7 8 9 1
1	0.28114	| . |******. |
2	0.08603	| . |** . |

NRF REVIEW

ARIMA Procedure

Conditional Least Squares Estimation

Parameter	Estimate	Approx. Std Error	T Ratio	Lag
MU	4.60116	1.00943	4.56	0
AR1,1	0.25666	0.18198	1.41	1
AR1,2	0.08930	0.18411	0.49	2

Constant Estimate = 3.00931497

Variance Estimate = 15.0645196
Std Error Estimate = 3.88130385
AIC = 186.012003*
SBC = 190.501525*
Number of Residuals= 33
* Does not include log determinant.

ARIMA Procedure

Correlations of the Estimates

Parameter	MU	AR1,1	AR1,2
MU	1.000	-0.004	-0.041
AR1,1	-0.004	1.000	-0.283
AR1,2	-0.041	-0.283	1.000

NRF REVIEW

ARIMA Procedure

Autocorrelation Check of Residuals

To Lag	Chi Square	DF	Prob	Autocorrelations					
6	2.79	4	0.593	-0.006	-0.015	0.083	0.006	-0.017	-0.242
12	4.74	10	0.908	0.135	-0.052	0.040	-0.007	-0.128	-0.035
18	10.50	16	0.839	-0.210	-0.027	0.064	-0.165	-0.121	-0.010
24	13.15	22	0.929	0.061	0.019	-0.112	0.084	0.052	0.018

ARIMA Procedure

Name of variable - DIFF.

Mean of working series = 0.607576
Standard deviation = 3.445018
Number of observations = 33

Autocorrelations

Lag	Covariance	Correlation	-1 9 8 7 6 5 4 3 2 1 0 1 2 3 4 5 6 7 8 9 1
0	11.868146	1.00000	|**********|
1	1.055075	0.08890	|. **. |
2	-1.569459	-0.13224	|. ***. |

"." marks two standard errors

NRF REVIEW

ARIMA Procedure

Inverse Autocorrelations

Lag	Correlation	-1 9 8 7 6 5 4 3 2 1	0	1 2 3 4 5 6 7 8 9 1
1	-0.11239	. **		.
2	0.13711	.	***	.

Partial Autocorrelations

Lag	Correlation	-1 9 8 7 6 5 4 3 2 1	0	1 2 3 4 5 6 7 8 9 1
1	0.08890	.	**	.
2	-0.14126	. ***		.

NRF REVIEW

ARIMA Procedure

Conditional Least Squares Estimation

Parameter	Estimate	Approx. Std Error	T Ratio	Lag
MU	0.61139	0.59930	1.02	0
AR1,1	0.10146	0.18074	0.56	1
AR1,2	-0.14159	0.18095	-0.78	2

Constant Estimate = 0.63592326

Variance Estimate = 12.6927637
Std Error Estimate = 3.56269051
AIC = 180.358765*
SBC = 184.848287*
Number of Residuals= 33
* Does not include log determinant.

Case No.: I.95-05-047
Exhibit:
Witness: Gregory M. Duncan
Date:

GTE CALIFORNIA INCORPORATED

REPLY TESTIMONY OF DR. GREGORY M. DUNCAN

Q. Dr. Duncan, what is the purpose of your reply testimony?

A. The purpose of my reply testimony is to rebut certain conclusions stated in the direct testimony filed by Dr. Lee Selwyn on behalf of the California Committee for Large Telecommunications Consumers (CCLTC).

Q. Have you reviewed the direct testimony Dr. Selwyn?

A. Yes. Dr. Selwyn agrees with most of the principles relied upon by Dr. Christensen. However, in contrast to Dr. Christensen, he states that there is a differential between the U.S. input price growth and the local exchange carrier (LEC) input price growth on a going forward basis. In stating this, he relies on a study performed by C. Anthony Bush and Mark Uretsky entitled "Input Prices And Total Factor Productivity" (hereafter "Bush-Uretsky") which appeared as Appendix F in the Federal Communications Commission's (FCC) First Report and Order released April 7, 1995 in CC Docket No. 94-1.

Q. Do you agree with the Bush-Uretsky analysis?

A. No.

Q. Please explain why.

A. Bush-Uretsky claim to have found a long run structural change in the relationship between the LEC input price series and the U.S. input price series. If this claim were true, it would overturn accepted economic fact in two

areas: (1) the microeconomic principle that markets clear, i.e., that input prices in different sectors of the economy must grow at the same rate except for random fluctuations; and (2) the macroeconomic principle that nominal price series are cointegrated, i.e., that they grow at roughly the same rates, differing only by short run random fluctuations. I discussed this at length in my direct testimony at pages 5 through 8. In fact, what Bush-Uretsky discovered was a sequence of irrelevant statistical artifacts which resulted from their misapplying statistical techniques (e.g., testing the wrong hypotheses, use of endogenous explanatory variables, and misuse of dummy variable techniques).

Q. How did Bush-Uretsky test the wrong hypothesis?

A. The question at hand is whether or not the U.S. LEC input price series deviates from the overall U.S. input price series in the long run. In point of fact, Bush and Uretsky test an entirely different and irrelevant hypothesis: that of whether the relationship between these two series and Moody's Yield On Public Utility Bonds series (hereafter "Moody series") showed any change since divestiture.

Bush and Uretsky postulated two relationships between LEC input price changes, U.S. input price changes and Moody's yields on public utility bonds. One relationship was between LEC input prices, the U.S. overall price index and the Moody series. The other relationship was between the differential between the two price input series and the Moody series.

Bush and Uretsky's first hypothesis was that the LEC input price change is a linear combination of the U.S. input price series and the Moody series, and that this relationship changed. Their second hypothesis was that the price differential is a linear function of the Moody series and that this relationship changed.

Their finding that there is some evidence that there has been a structural change in both relationships is in error as will be shown below. More importantly, it is totally irrelevant. The relationship between baseball ticket prices and LEC input prices has also changed since divestiture; however, such findings tell us nothing about whether there has been a structural change in the relationship between the two input price series themselves.

Q. You mentioned two other errors in addition to testing the wrong hypothesis. What were these?

A. The first other error is the endogeneity of both the U.S. input price series and the Moody series. An endogenous variable cannot be used as an explanatory variable, but Bush-Uretsky in fact use both as explanatory variables. The reason they are endogenous variables is that they both reflect and are reflected in changes in the LEC input price series. Therefore, these variables must be correlated with the error in the equation, which violates a fundamental requirement for valid regression analyses.

Q. Can this error be corrected?

A. Yes, and in the process, correction of this error

will also eliminate the error previously described, i.e., testing the wrong hypothesis. These errors can be corrected by dropping the Moody's variable from the regression equation and concentrating on the long run stability of the difference in the price series.

Q. What is the remaining other error?

A. Yes. The final irremediable error is misuse of dummy variable methodology. Let us for a moment ignore the introduction of the Moody's Yield on Public Utility Bond series, which as explained above is endogenous and biases their results about the stability of the relationship. Let us consider introducing dummy variables to test for changes in structure. While such procedures, properly employed, have a long and happy history, improperly employed, they muddy thinking and yield incorrect results.

There are hard rules for performing analysis using dummy variables. Among these is the rule that you cannot look at the data before you decide where the structural break occurred. Another rule is that either there must be a theoretical reason for specifying the structural break at the point where the dummy variable is introduced, or an empirical reason arrived at by examining a wholly independent set of data.

Q. You mean you cannot look at your data before deciding which hypothesis to test?

A. That is correct. To do so leads to a never ending sequence of adding dummy variables. There is an old story

among time series specialists that goes this way. A famous statistician took a set of random numbers and plotted them against time. He then told students that there was a nonrandom pattern in them which could be found. Most of the students found a pattern. The statistician's point was that if you go mining for a result in data, even random data can be made to give it. That is why it is so important to have a theoretical basis for a hypothesis and to ensure the hypothesis is validated on more than a "drop this observation, add that observation" basis.

Taking this a little further, if one were to look at the random pattern and "find" a pattern, and insert a dummy variable to account for the pattern, then a test of whether the dummy variable was significant would always be passed. For example, let us say some one finds a positive price differential near the end of a random series, they insert a dummy variable, and find that the coefficient is, say, 2.7. To test this hypothesis one cannot use the same set of data. Instead, one must generate another set of data from the same process, and look at the last corresponding observations. One would test whether these observations had the same 2.7 mean as in the first series.

In the Bush-Uretsky method, to test their hypothesis that economic theory is wrong about input prices equalizing across sectors, and the difference between the LEC input price series and the U.S. economy input price series will persist, they must now either wait 10 to 15 years to see if their

hypothesis is borne out in the LEC industry, or they must look at a random sample of other sectors and see if in those sectors' prices are adjusting differently than the overall economy input prices. They did neither and in fact proceeded to misuse classical statistical analysis. They fell into the trap of looking for patterns in all the wrong ways.

Q. What did they do?

A. They introduced a dummy variable that attempts to account for the time since divestiture and regressed the LEC series on the U.S. series, the bond price series and the divestiture series. They found a statistically significant effect of divestiture and concluded that the series are different.

Q. Doesn't that prove their point?

A. No. All their finding says is that the relationship between the Moody series and the price differential series has changed. They cannot conclude from this that the two price series grow at different rates in the long run or that any observable differences in the series are anything but completely random.

Q. How should a proper test be performed to see if the series are the same?

A. There are many ways. For example, the analyses performed by Christensen and NERA were one way of performing such a test. I myself would take a different but equivalent approach.

First, I would work with the difference between the

two price series and see if there is any evidence of long run deviation. The simplest way to do this is to do a time series analysis of the difference in the series to see if the series is both stationary and has a zero mean. This is what I did in my direct testimony. If either is lacking, then we might be suspicious that the two series forming the difference grew at different rates. Of course, as I discussed above, such a finding would be stunning.

Such a finding would suggest overturning two whole areas of economics: one that says factor markets equilibrate across output sectors, and consequently, input prices facing producers in one sector, are in the long run, the same as input prices facing producers in another sector, which has the further consequence that the input prices in any sector mimic the input prices in the economy as a whole. The second one says on a macroeconomic level that nominal prices in all sectors should be cointegrated, that is, except for short run deviations, all prices will grow at more or less the same rate, although the rate itself may vary over time.

Q. Didn't Bush and Uretsky do this?

A. No. While they did look at the differential between the two price series, they committed the same two errors as above. First, they investigate whether there is a stable relationship between the differential input price series and the Moody series; and second, they engage in a game I call "find a place for the dummy variable."

Q. Can you give specific examples of this game using

their data?

A. Yes. Bush-Uretsky chose to break the data at 1984, the year of divestiture. Of course, one could argue as easily, the change was anticipated and the market reacted in 1983, so that the break should happen then. If you put the break at 1983, eliminate the endogenous Moody series as an explanatory variable, and test that the pre-divestiture data and post-divestiture data are the same, you cannot reject the hypothesis that markets clear, that is that the series move the same way.

Similarly, one might argue that there was a short-run deviation in 1984 through 1988, but that by 1989 the market had adjusted to its new equilibrium and things were back to normal. To test this hypothesis you would introduce two dummy variables, one for the 1984 through 1988 period and one for the 1989 through 1992 period. You would then test whether the 1989 through 1992 period was different than the pre-divestiture period.

Finally, one might break the periods at half decades. For example, one might introduce dummies for the first and last parts of each decade since 1970 on the grounds that the technological change in the industry started in 1970, shortly after the Carterfone decision, and that prices fluctuate in five year cycles, according to five year planning periods. Then one would expect the LEC input price series growth to first be higher than the U.S. series as industry geared up to accommodate competition, then for it to be lower,

and then to settle down. This would show itself by having an insignificant 1975 through 1979 dummy because no one anticipated competition, a negative 1980 through 1984 dummy as the market geared up for competition, a positive 1985 through 1989 dummy as the market begins to shake out and an insignificantly different from zero dummy for the 1990 through 1992 period as things return to normal.

Q. Have you conducted these tests?

A. Yes.

Q. And were your suppositions supported?

A. Yes. But let me preface telling you about them by saying in performing these tests I am committing the same error I accuse Bush-Uretsky of: that of inserting a dummy variable and testing its effect with no supporting underlying theory or independent theoretical result.

In Attachment R1, I perform a test of the hypothesis that the 1983 through 1992 period was different from the 1960 through 1982 period. The t-statistic on the D83 variable is .993 indicating there is no evidence to overturn two pillars of economic thought, that markets clear.

In Attachment R2, I perform a test of the hypothesis that the data return to normal by 1989. I do this by regressing the input price series difference on two dummy variables: one for the 1984 through 1988 period, and one for the 1989 through 1992 period. A t-test on coefficient on the 1989 through 1992 dummy, D89, cannot deny that the price series have returned to a zero difference. The t-statistic on

that test was .778.

Finally, in Attachment R3, I test the hypothesis that the 1990 through 1992 period is the same as the 1960 through 1980 period. Again, a t-test on the 1990 through 1992 dummy cannot deny that the 1990 through 1992 period is the same as the 1960 through 1980 period. The t-statistic for this test is -1.051. In all of these tests I used the Bush-Uretsky data, even though I am skeptical of their methodology for obtaining the U.S. price series.

Q. Don't your results show a positive differential through the 1984 through 1989 period and doesn't this support the hypothesis relied upon by Bush-Uretsky?

A. No. At best it indicates there was a statistically insignificant short run aberration in the difference, probably due to markets adjusting to eliminate the difference.

Q. Well, shouldn't that be adjusted for in the "x" factor?

A. Absolutely not. To do so means that the California Public Utilities Commission is reacting to the noise in the system. Any quality control engineer will tell you that you do not respond to noise, only real and permanent changes in structure. The same is true for economic systems. Responding to noise gains nothing, is expensive, and may destroy the system.

In fact, looking at Attachment R3, it shows the LEC input price growing faster than the U.S. input price index. However, this result is not significantly different from zero,

so adjusting the "x" factor downward, as would be consistent with Dr. Selwyn's flawed approach, though it would benefit us, is uncalled for. To do so would simply be responding to noise as Dr. Selwyn has.

Q. What then can we conclude about the use of the Bush-Uretsky results in determining whether the LEC input price index differs from the U.S. input price index by more than random fluctuations?

A. We can conclude nothing from their analysis because of the errors discussed above. The properly done analysis is the analysis presented in my direct testimony. From that analysis, we can conclude that there is no long run differential between the series and as a consequence there should be no input price adjustment to the "x" factor. Further, the Christensen study can be accepted in totality as a basis for calculating an "x" factor (if the Commission persists in its reliance on an "x" factor).

Q. Does this complete your testimony.

A. Yes it does.

NRF REVIEW: ATTACHMENT R1

Autoreg Procedure

Dependent Variable = DIFF

Ordinary Least Squares Estimates

SSE	379.308	DFE	31
MSE	12.23574	Root MSE	3.497963
SBC	181.2237	AIC	178.2387
Reg Rsq	0.0815	Total Rsq	0.0815
Durbin-Watson	1.8508		

Variable	DF	B Value	Std Error	t Ratio	Approx Prob
Intercept	1	0.20634785	0.7284	0.280	0.7812
D83	1	1.39065217	1.3258	1.004	0.3250

Estimates of Autocorrelations

Preliminary MSE = 9.858876

Estimates of the Autoregressive Parameters

Lag	Coefficient	Std Error	t Ratio
6	0.37816883	0.16901558	2.237482

NRR REVIEW: ATTACHMENT R1

Yule-Walker Estimates

SSE	298.3924	DFE	30
MSE	9.946415	Root MSE	3.153794
SBC	177.7282	AIC	173.2387
Reg Rsq	0.0818	Total Rsq	0.2381
Durbin-Watson	1.8787		

Variable	DF	B Value	Std Error	t Ratio	Approx Prob
Intercept	1	0.24137214	0.5122	0.471	0.6403
DES	1	1.03625924	1.0434	0.993	0.3286